This paper presents a simple inverse technique to deduce bedrock topography given an ice flow model, surface mass balance distributions in space and time, and modern surface elevation data. The approach is very simple and iterative, iteratively running the model forward to the present, and adjusting bed topography locally in proportion to the surface elevation difference from observed (ignoring the non-local nature of dynamics). The technique is shown to work well in synthetic tests, with interesting convergence properties. Then it is applied to the Nordenskioldbreen glacier, Svalbard, where results are validated against limited radar tracks of bedrock topography. The procedure also produces a reasonable spun-up modern state of the ice model that can be used to initialize future experiments.

The paper is clearly written, and proceeds nicely from simple concepts and idealized tests to the more complex real glacier setting and experiments. The method, although simple, is new to my knowledge; its simplicity will make it very amenable to other groups modeling glaciers where bed topography is largely unknown. The paper shows clear improvement over the much earlier perfect-plasticity method (pg. 894-5, Fig. 13). Also, it shows how validation versus radar bed information can also constrain the best-fit till-strength value in the basal sliding parameterization (Fig. 12). Overall the paper is an interesting and valuable contribution, and in my opinion needs only minor revisions, assuming the answer to the last question in point #1 is positive.

Specific points

1. The paper addresses the possible influence of the initial bed construction on the reconstructed bed (pg. 892-893). This is tested to some extent by Fig. 11 column 3, where the initial bed is lowered by 100 m. But in all runs, even with “unconstrained beds” (Figs. 9 et seq.), the initial bed is still constructed using the GPR radar data (pg. 887, lines 11-13), as can be seen in Fig. 9 for n= 1, where the over-deepening mentioned on pg. 891 line 27 is already in the initial bed. It would be preferable to avoid any influence of the validation data in these runs at all, even in initialization. Could a more radical perturbation to the initial bed be tested, such as constructing it with the GPR data ignored? Would the inverse procedure still produce the over-deepening?

2. This is mostly a comment. As discussed in the paper, surface elevations also depend strongly on uncertain bed sliding properties (here mostly encapsulated in ϕ, the material till strength). This under-determination is handled well, simply by trying different uniform values of ϕ (Figs. 11, 12). But even with uniform ϕ there are probably large spatial variations in sliding due to water pressure p_w in Eq. (1). p_w in turn depends on basal water amount W predicted by the model. Presumably there are regions under Nordenskioldbreen with essentially no sliding, where W is almost zero and/or the bed is frozen, and these regions have significant effects on ice thickness. (Perhaps that is why internal deformation is dominant in the interior, and sliding near the margins; pg. 881, line 7-9). The distribution of W~0 or frozen-bed areas can be regarded as another model source of uncertainty in the results, as discussed in general in the paper. But it is an important one, with potentially large effects on ice thickness in the forward model, and thus on the deduced bed topography in the inverse procedure.

Another paper (van Pelt and Oerlemans, 2012, referenced here) focuses on these aspects in a synthetic setting. Given that, the single sensitivity test over a range of ϕ (Figs. 11, 12) seems sufficient, especially since Fig. 12 suggests the results worsen for ϕ outside the range. But it would be of interest to add a figure(s) of the distribution of W, p_w and/or τ_c, mainly to show the areas of frozen/dewatered bed with no sliding.

Also, following Eq. (1), it would help to give a bit more information on the treatment of basal water in PISM. The formulae for p_w and W are given in van Pelt and Oerlemans (2012), but they could be repeated here, or at least described verbally.

3. The discussion and implementation of the L-curve stopping criterion (e.g., Fig. 13) is valuable. But in the synthetic experiments, which are stopped after n=40 in Figs. 2-4, it would be interesting to know what happens if the iterations are continued much longer. Is there any further reduction of the error from the actual bedrock bump, shown in the last panels, or is there little change after n=40 ?

4. The importance of prescribing realistic time history of climate forcing is demonstrated (Fig. 11 column 4), from 1300 AD to modern (from 1598 AD for precipitation), as described in section 4.3. But the climate prescription for the earlier part of the runs (500 AD to 1300 AD) seems quite casual in comparison (pg. 886, line 6-7), set constant to the mean after 1300 AD. Presumably this is because there is no comparable data available before 1300 AD. But could a sensitivity experiment be done to show whether different but still reasonable choices of climate for 500-1300 AD significantly affect the results?

5. pg. 899, line 10 (and abstract line 18-20): The discussion of applicability to "larger sets of glaciers and ice caps" could be amplified. As it is, the concept is not very clear. Does it mean that a large number of glaciers could each be treated separately by the procedure as in the paper, and the individual results summed? Or that the input properties (surface profiles, surface mass balance) of a large set of glaciers be averaged, and the procedure applied once to that, yielding just one regional result?

6. The recent paper by De Rydt et al., The Cryo., 2013, could be mentioned, which supports the theoretical results in Gudmundsson (2003) and Raymond and Gudmundsson (2005) with field data analysis; the latter 2 papers are referred to several times here.

Technical points

a. pg. 880, line 18-19: Why are there multiple layers in the bedrock? Does the model simulate vertical heat transfer and temperature profiles in the bedrock?

b. Vertical bed deformation due to ice loading is presumably neglected here, as appropriate for small glaciers. But it could be significant for larger ice masses and longer time scales. If the model includes a bed-deformation module, in principle it could be included and the inversion procedure would operate on the prescribed ice-free equilibrated bed.

c. pgs. 882-883: Specify the bump size for the reference bed in Fig. 3, and the two sizes in Fig. 5 (like 150 m for Fig. 2, on pg. 882 line 1).

d. pg. 885, line 25, and/or Fig. 6 caption: Along with the names of the exposed-rock areas, say that is what they are (or in other words, such as mountains, nunataks?), for readers unfamiliar with “fjellet”.

e. pg. 886, lines 22-27: The descriptions of how the procedure handles discrepancies between model vs. observed ice perimeters (around enclosed exposed-rock areas), and also how the divide is handled, are a little unclear. In line 22, for points in the immediate vicinity of the divide, is “nearest-neighbor interpolation” from points inside the glacier domain, or outside (with the latter set as on pg. 887, line 16-18)? And does “immediate vicinity” (line 21) mean exactly on or touching the divide? For (1) and (2) in lines 24 and 25, “interpolating” between or from what, and is this still nearest-neighbor or other type of interpolation?

Presumably these steps would also be taken for other glaciers with land-terminating margins (not the case here, where the calving front is held static through the runs as mentioned on pg. 887, line 1-2).

f. In later sections where sensitivity results are compared to the standard run with ϕ=13, the term “bed misfit” is confusing, e.g. Fig. 11 and 15 captions. It is a sensitivity to a changed model parameter, not a misfit from observations. (Except in Fig. 5(d) caption, where the term is appropriate - the misfit from the known synthetic bed).

g. pg. 888, line 22: Probably “paragraph” should be “section”.